MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

12. *Suspended Animation.*—The Royal Medical and Chirurgical Society some time since appointed a committee to investigate and report on this subject. This committee has recently (July 1) made an elaborate and highly interesting report, of which we lay before our readers a brief summary.

The inquiry was conducted—By means of experiments upon living animals; by means of experiments upon the dead human body. In investigating anew the subject of apnœa by means of experiments on the lower animals, it seemed expedient to observe, in the first place, the principal phenomena of apnœa in its least complicated form—namely, when produced by simply depriving the animal of air. The principal facts to which attention was directed during the progress of the apnœa thus induced were—The duration of the respiratory movements; the duration of the heart's action. The duration of the heart's action was observed—(*a.*) In relation to the duration of the respiratory movements. (*b.*) In relation to the time after the stoppage of the breathing. From the experiments performed, it appeared that in the dog the average duration of the respiratory movements after the animal has been deprived of air is 4 min. 5 sec., the extremes being 3 min. 30 sec. and 4 min. 40 sec. The average duration of the heart's action is 7 min. 11 sec., the extremes being 6 min. 40 sec. and 7 min. 45 sec. From these experiments, it appears that on an average the heart's action continues for 3 min. 15 sec. after the animal has ceased to make respiratory efforts, the extremes being 2 min. and 4 min. respectively. Rabbits on an average ceased to make respiratory efforts in 3 min. 25 sec. Their heart's action stopped in 7 min. 10 sec.; consequently the interval between the last respiratory effort and the cessation of the heart's action was 3 min. 45 sec.

The next question investigated was—The period after the simple deprivation of air at which recovery is possible, under natural circumstances, without the aid of any artificial means of resuscitation. The experiments performed led to the conclusion that a dog may be deprived of air during 3 min. 50 sec., and afterwards recover without the application of artificial means; that a dog is not likely to recover, if left to itself, after having been deprived of air during 4 min. 10 sec. The force of the inspiratory efforts during apnœa was observed in the experiments to be so great that it was determined to measure them. They were found to be capable, in the dog, of raising a column of mercury four inches. It appeared, moreover, that their force increases up to a certain period. In other experiments, plaster of Paris, and even mercury, were thus drawn upwards into the minute bronchial tubes. It is easy to understand, therefore, how foreign bodies may be drawn into the lungs in cases of drowning, and the importance of this fact in the consideration of the pathology and treatment of apnœa.

The committee next passed on to the subject of drowning. The first question investigated was—For what period can an animal be submerged, and yet recover without the aid of artificial means? It was found as the result of numerous experiments on dogs that, in striking contrast to the previous ones, one and a half minute's immersion in water suffices to destroy life. Other experiments satisfactorily showed that the difference of time between simple apnœa and that by drowning is not due to submersion, or to depression of temperature, or to struggling, but that it is connected with the fact that in the one case a free passage of air out of the lungs, and of water into them, is permitted; in the other, the exit of air and the entrance of water are prevented. There can be no doubt, from other considerations put forward, that although both these circumstances are concerned in producing the difference observed, yet that it is mainly due to the entrance of water and the effects thereby produced.

The treatment of apnœa was next considered. For conclusions respecting artificial respiration the Committee refer to the second portion of the Report. Many other methods of resuscitation which have been recommended were employed, including actual cautery, venesection, cold splash, alternate application of hot and cold water, galvanism, puncture of the diaphragm. Although some of the above means were occasionally of manifest advantage, no one was of such unequivocal efficacy in a sufficient number of cases as to warrant the Committee in specially recommending its adoption. The experiments upon the dead subject were made with a view to determine the value of the various methods which have been employed for alternately compressing and expanding the cavity of the chest in such a manner as to imitate the natural movements of the thoracic walls in breathing. The following methods have been investigated: 1. Pressure exerted by the hands on the anterior wall of the thorax, the body being in the prone posture. Such pressure has for its object to expel a portion of the air contained in the chest; on relaxing the pressure the chest expands and air enters. 2. The postural or so-called "ready" method, described by Dr. Marshall Hall, which consists essentially in "turning the body gently on the side and a little beyond, and then briskly on the face alternately;" and in making pressure along the back of the chest each time the body is brought into the prone position. 3. The method of Dr. Silvester, in which the action of the pectoral and other muscles passing from the shoulders to the parietes of the chest in deep inspiration is imitated. An inspiratory effort is produced by extending the arms upwards by the sides of the head; on restoring them to their original position by the side of the body, the expanded walls are allowed to resume their previous state, and expiration takes place, the quantity of air expelled being in proportion to that which had been previously inspired. It being necessary to measure the flow of air in and out of the respiratory cavity under conditions of pressure closely resembling those which exist in natural respiration, no means of measurement could be used which, in its working, would offer any appreciable resistance to the passage of air. With this consideration in view, an instrument designed by Dr. Sanderson was employed. (The instrument was exhibited to the Society.)

General results.—1. As regards the volume of air which can be expelled from the thorax by compression of its walls, and inspired by the elastic expansion consequent on relaxation of the pressure, it was found—(*a.*) That pressure by both hands on the lower third of the sternum in the adult male subject usually displaced from eight to ten inches of air. The pressure actually exerted amounted to about thirty pounds. It was, therefore, not greater than might be safely applied to the living subject. The volume of air expelled varied from eight cubic inches to fifteen cubic inches. (*b.*) That pressure made in the same manner on the upper part of the sternum usually displaced two or three cubic inches less than pressure on the lower part. (*c.*) That pressure exerted by one hand on the upper part, by the other hand on the lower part of the sternum, produced about the same results as were observed in *a*. In this case the whole amount of pressure did not exceed that exerted in *a*. (*d.*) That the pressure of a weight laid on the lower third of the sternum produced similar results according to its amount. (*e.*) The lateral pressure exerted on the ribs or costal cartilages of both sides simultaneously was in no instance more effectual. (*f.*) That compression by a broad bandage encircling the chest, the ends of which were crossed over the sternum, and drawn in opposite directions by two persons, produced no greater effect than pressure with the hands on the sternum or sides. 2. As regards the whole amount of exchange of air produced by the method of Dr. Marshall Hall, "to imitate respiration," it varied much, according as the subject was favourable or the contrary; sometimes not exceeding a few cubic inches, but never exceeding fifteen cubic inches. 3. As regards Dr. Silvester's method, it was found that, on extending the arms upwards, a volume of air was inspired into the chest, which varied in different subjects, from nine to forty-four cubic inches, and it was observed that the results obtained in successful experiments on the same body were remarkably uniform, in which respect, as well as in their amount, they contrasted with those obtained by the method of Dr. M. Hall. On restoring the arms to the side, the quantity of air expelled was generally nearly equal to that previously inspired, occasionally less.

In the treatment of apnœa generally, the committee offer the following suggestions: That all obstruction to the passage of air to and from the lungs be at once, so far as is practicable, removed; that the mouth and nostrils, for example, be cleansed from all foreign matters or adhering mucus. That in the absence of natural respiration artificial respiration by Dr. Silvester's plan be forthwith employed in the following manner: The body being laid on its back (either on a flat surface, or, better, on a plane inclined a little from the feet upwards), a firm cushion or some similar support should be placed under the shoulders, the head being kept on a line with the trunk. The tongue should be drawn forward so as to project a little from the side of the mouth. Then the arms should be drawn upwards until they nearly meet above the head (the operator grasping them just above the elbows), and then at once lowered and replaced at the side. This should be immediately followed by moderate pressure with both hands upon the lower part of the sternum. This process is to be repeated twelve or fourteen times in the minute. That if no natural respiratory efforts supervene, a dash of hot water (120° Fahr.) or cold water be employed, for the purpose of exciting respiratory efforts. That the temperature of the body be maintained by friction, warm blankets, the warm bath, etc.

In the case of drowning, in addition to the foregoing suggestions, the following plan may be in the first instance practised: Place the body with the face downwards, and hanging a little over the edge of a table, shutter, or board, raised to an angle of about thirty degrees, so that the head may be lower than the feet. Open the mouth and draw the tongue forward. Keep the body in this posture for a few seconds, or a little longer if fluid escapes. The escape of fluid may be assisted by pressing once or twice upon the back. (Signed)—C. J. B. Williams, *Chairman;* W. S. Kirkes, George Harley, J. B. Sanderson, C. E. Brown-Séquard, H. Hyde Salter, E. H. Sieveking, *ex officio;* William S. Savory, *Hon. Sec.*

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13. *Two Cases of Extensive Arterial Obstruction from Separated Cardiac Vegetations, followed by Gangrene and Death.*—Dr. GOODFELLOW read before the Royal Medical and Chirurgical Society (June 24) two cases of this character. The extent to which the plugging took place, the number of vessels involved, the morbid changes in and around the coats of the vessels at the seat of obstruction, and the consequences which ensued, appeared to the author to give a peculiar interest to them. In both cases vegetations of considerable size had formed on the mitral valve and surrounding surface of the endocardium. Some of these had become detached, and caused obstruction to the circulation in several of the large arterial trunks; coagula formed around them, and complete occlusion followed. The symptoms were well marked—namely, pain, intense and agonizing, at the seat of obstruction, and coldness and numbness at the distal extremities of the affected limbs, speedily followed by dry gangrene. In the first case the evidences of occlusion were observed about a month before the fatal event, and about seven days prior to the appearance of gangrene. In the second case the interval between the evidence of obstruction and the appearance of dry gangrene was shorter; the pathological changes in and around the walls of the arteries at the seat of obstruction were less extensive. The first case was that of a woman, aged 30, who had had an attack of acute rheumatism twelve years prior to her admission into the hospital. The heart was damaged during that attack. She, however, was enabled to follow her usual occupation, with occasional interruptions, up to a short period before the appearance of the symptoms denoting obstruction. The second case was that of a girl, aged 17. She had had an attack of acute rheumatism about three years before, complicated with pneumonia, but not with heart affection. Another attack of rheumatism occurred about eighteen months afterwards, which was complicated with endocarditis. From the time of this attack to the period of her seizure with her last fatal illness, she suffered considerably from dyspnœa and frequent and severe pain in the præcordial region.—*Med. Times and Gaz.*, July 5, 1862.

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14. *Epidemic of Typhoid Fever Dependent upon the Use of Impure Water.*—During the autumn of 1860, there prevailed in the convent of the Sisters of Charity, in Munich, an epidemic of abdominal typhus, which was the more remarkable because at that time there were only a very few isolated cases scattered through the town. From the 1st of June till the beginning of September, there

were only two cases of typhoid fever in the convent; but from the 19th of September till the 4th of October, when the population of the convent consisted of a hundred and twenty persons, *thirty-one* of the sisters became rapidly affected, one after the other. Some presented gastric symptoms, others were attacked with regular typhoid fever. Of the fourteen cases of typhoid fever, four proved fatal.

The attention of the medical profession was especially called to this state of matters, because the sanitary state of the town was at that time very satisfactory, and typhoid fever was very rare. It was evident, then, that the cause of this epidemic must be look for in local circumstances; and, after a careful examination, it was discovered that the water, used as drink by the inmates, was mixed with substances in a state of putrefaction, and that it constituted the cause of the epidemic. The local circumstances were the following: The convent is situated beside the general hospital. In the spring of 1860, a well was dug in the latter, having a depth of twenty-feet. This well was distant only two feet from the laundry in which the clothes of the patients were washed, and was surrounded by the openings of five drains, intended to absorb the water which ran off from the laundry, and connected with one another by gutters, from which the water was insensibly filtered into the surrounding soil. This water was turbid, had a disagreeable odour, and contained an abundant sediment. As the drains were only twenty or thirty feet from the well, the water in the latter was contaminated by that in the drains. To demonstrate this fact, the contents of the drains and the water of the well were submitted to a careful microscopic examination by Dr. Hessling, a skilful microscopist. He found, in the sediment of the water from the drains, various kinds of matter, of animal and vegetable origin, in a state of decomposition: some could still be recognized by their original forms, but the greater part constituted only a mass of detritus. This detritus was in the form of a flaky coagulum of a dark-green colour, forming little masses, some of which contained inorganic matters, such as sand, particles of lime, etc., which seemed to have constituted the nuclei. Both the water and the sediment contained a large quantity of carbonate of lime, and on the addition of a little sulphuric acid, a strong odour, resembling that of rotten eggs, was disengaged; the same phenomena took place even with the small objects which were submitted to microscopic examination. In addition, there were observed organic elements of new formation, such as algæ, spores, vibriones, and monades, which moved rapidly about in the field of vision. The water from the well deposited no sediment; nevertheless, on examining it microscopically, the same elements were observed as in the water coming from the drains, but in a very diluted state, especially as concerns the flaky coagula, the spores, and the vibriones.

It was consequently demonstrated that the water of the well had been contaminated by the contents of the drain. Professor Pettenkoffer, who made a chemical analysis of the water of the well, discovered in it a much larger quantity of organic matters, of lime, and of nitrates, than is contained in ordinary drinking water.

The water of the new well habitually supplied the requirements of the laundry. Between the 17th and 28th September, the time when the epidemic commenced, the water was conducted by pipes into the bath-room and kitchen of the hospital and convent, because, as repairs were being carried on in the bath-room, these establishments no longer received a sufficient supply of water. It was recommended that this water should only be employed for baths, for washing, and for the kitchen, and that the water which was to be used for drinking should be furnished from two wells situated in a court intermediate between the hospital and the convent. Nevertheless, although the attendants in the convent had been sufficiently warned, it appeared, after a careful investigation, that the water which was carried in the evening from the kitchen into the bedrooms of the sisters to be used for the toilette, had been also used for drinking; and all the persons who became ill, acknowledged that they had drunk this water.

If it be considered that the epidemic commenced at the very time when this water, which contained putrefying organic matters coming from the dirty linen of the patients, began to be drunk, we are authorized in concluding that the cause of the typhoid affection existed in the poisonous properties of the water—a conclusion which is still farther justified by the fact that the epidemic ceased as soon as the water in question ceased to be employed for drinking.—*Edinb. Med. Journal*, June, 1862, from *Nederlandsch Tijdschrift voor Geneeskunde.*

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15. *Diabetes with Spinal Irritation.* By Dr. E. A. KUNKLER.—The subject of this case, was a man, 26 years of age, who had been the subject of diabetes for seven months. Notwithstanding the use of morphia, iron, bitters, exclusively animal diet, &c., he got worse; the emaciation, thirst, want of sleep, frequent epistaxis being excessive. After consulting numerous physicians without benefit, the relater of the case, bearing in mind the hypothesis of Scharlan, that the origin of the disease was to be found in the vertebral column; and the discovery by Bernard, that irritation of the fourth ventricle of the brain caused the appearance of sugar in the urine and blood, thus connecting the disease with some derangement of the nervous system; and moreover finding that in this case the superior part of the dorsal portion of the spine was tender at many points, applied a certain number of cupping-glasses. Their application was followed by a disappearance of this morbid sensibility, but there was no abatement of the disease itself. Immediately, however, after a blister upon the neck, the presence of sugar and the unusual excretion of the urine ceased most unexpectedly, as if by enchantment. A slightly disagreeable sensation, which hitherto the patient had experienced at the lower part of the brain, also ceased at once. After continuing for ten or twelve days blisters upon the neck and behind the ears alternately, the affection had entirely and permanently disappeared, and this in spite of a varied diet.

The particulars of the above case were sent to M. Bernard, with the hope that as Paris offered more cases of diabetes than the locality where Dr. Kunkler lived (Placerville, California), he would give the plan of treatment an extended trial.

The quoter of the case in *L'Union Médicale* recalls to notice the recital by Becquerel of an instance in which glucosuria occurred in a case of acute myelitis, and in a case of spinal meningitis with tumour of the pia mater and softening and cyst of the cerebellum; also by Lebert, of diabetes in a hemorrhagic affection of the spinal cord; and by Scharlan, of the post-mortem examination of two cases of diabetes, in which softening of the cord, with other changes, were found. He wisely suggests that the cure of diabetes by the means above related, must not always be looked for, inasmuch as although generally being developed under the influence of the nervous system, its modes of origin are very various. —*B. and F. Med-Chir. Rev.*, July, 1862, from *L'Union Méd.*, July 29, 1861.

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16. *Diphtheria and its Sequelæ.*—Dr. JAMES BEGBIE has published (*Edinb. Med. Journ.*, May, 1862) some interesting observations on diphtheria. His cases tend to confirm the views generally entertained in regard to the nature of diphtheria, and go to establish:—

1*st*. That it is a constitutional disorder having the character of fever, running a definite course, and bearing a closer affinity to scarlatina and typhoid fever than to any other specific disease.

2*d*. That its local manifestation is chiefly observed on the mucous membrane of the mouth and throat; the tonsils, uvula, and palate with the pharynx being first affected; but that it has a tendency to spread to the adjoining passages, and is particularly prone to invade the larynx.

3*d*. That this local disease is of the nature of inflammation of asthenic character, with exudation of lymph in the form of pellicle.

4*th*. That the disease is contagious, and that youth and consanguinity powerfully predispose to it.

5*th*. That it is fatal from the severity of the general disorder, or from the exudative inflammation invading the larynx, and causing suffocation; or that death may result from the nervous disorder supervening in the form of paralysis.

And, lastly, that as we have no specific remedy for diphtheria, the disease and its sequels must be treated on the general principles which regulate our practice in fever and in inflammation, and in nervous disorders of asthenic character.

17. *Influence of Temperature on Yellow Fever.*—Dr. Archibald Smith (for many years resident in Peru) stated at a late meeting (June 2) of the Epidemiological Society, that he had noted all the essential symptoms of yellow fever at an elevation of 11,250 feet, with a temperature in the wet season of 62° within doors, with little variation day and night. At this temperature the disease lost none of its energy. In 1853 yellow fever appeared simultaneously on both sides of the Andes, and in 1854 assumed its most malignant character as well by the seaboard as on the hill-land. It was shown that these epidemics were of one generic nature. The yellow fever symptoms became modified gradually into the typhous or typhus, in the transit from the Pacific shores to higher and still higher regions of the Andes. In the warmer inland valley, as, *e. g.*, in the sugar-growing district of Abancay, the fever, which near the snows of the Cordilleras was metamorphosed from the typhus-icterodes of the coast into a form which, in a great measure, represented ordinary British typhus, was again reinstated with its most aggravated coast symptoms of yellow fever, such as intense frontal headache, dark sanguineous vomiting or evacuations, subcutaneous hemorrhage in form of large, dark maculæ, nasal hemorrhage, intense yellow colour of the skin, and the most extreme prostration of vital forces. In the Sierra it was propagated slowly from place to place, and from person to person. It was all but incurable in the dark and crowded huts, but yielded readily in a great majority of cases to early treatment, under the advantages of pure air and hygiene. Left to itself in the hovels of the Indian poor, it was prodigiously fatal.—*Med. Times and Gaz.*, July 19, 1862.

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18. *Clinical Researches upon Auscultation of the Head.* By Henry Roger, M. D., Prof. Fac. Med. Paris.—It is well known that the first application of the discovery of Laennec to the diagnosis of diseases of the brain was made by Dr. Fisher, of Boston. In 1833 this physician communicated to a learned society a memoir upon the *cephalic bellows sound*, and transmitted his interesting researches to the *American Journal of Medical Science*, Aug. 1838. "Auscultation," said Dr. Fisher, "may be as useful in the symptomatology of affections of the brain as it is in those of the chest, and may furnish a pathognomonic sign of these diseases."

After having traced the rules of cerebral auscultation, and indicated the normal sounds which reach the ear when applied to the head, and having distinguished those which come from the nasal fossæ, and which belong to respiration and phonation, as well as those which proceed from deglutition and from the head itself; after having distinguished all these from abnormal sounds, Dr. Fisher considers the *cephalic souffle* in diseases of the encephalon. He mentions, among other affections of which an abnormal bruit is one of the symptoms, *chronic hydrocephalus, cerebral congestion*, either simple or resulting from disordered dentition or hooping-cough, *acute inflammation of the encephalon or its membranes, abscess* of the brain and *induration* of that organ.

Dr. Whitney went even further than his colleague in his opinions with regard to the symptomatological value of the cephalic souffle (*American Journal of Medical Science*, Oct. 1843). To the affections mentioned by Dr. Fisher—cerebral congestion, meningitis, &c., in which the stethoscopic phenomena were present, he adds scirrhous transformation of the cerebellum, and mechanical compression of the brain. He says, moreover, that he perceived a *cerebral ægophony* in hydrocephalus, a bruit similar to the *catarrhal fremitus* in aneurism of the arteries at the base of the brain, and finally he points (and with reason, in certain cases) to the existence of a *cephalic souffle* in *anæmia of the brain* and also in *chlorosis*.

Notwithstanding the labours of these two American authorities, each of whom confirmed the observations made by the other, and in spite of results which generally agree, and which seem to have the testimony of numbers—for the facts in their memoirs count by hundreds—cerebral auscultation, which promised to throw an unexpected light upon a very obscure point in pathology, met with but little favour. The European medical journals confined themselves to incomplete analyses and short extracts; while but few practitioners thought it their duty to repeat the experiments necessary for a criticism of the facts which had been stated by our American brethren, as if they mistrusted a discovery coming from such a distance.

In the year 1841, in the first edition of the *Traité Pratique d'Auscultation*, mention was made of the observations of Drs. Fisher and Whitney; but the experiments to which M. Barth and I devoted ourselves for testing their truth, though certainly not very numerous, only gave us negative results. After having searched in vain to discover a cephalic souffle in certain cerebral affections, and having been unable, in twelve or fifteen cases of meningitis, to hear any such, while the American doctors declared it to be present in all; strengthened besides in our scepticism by the silence maintained by MM. Andral and Bouillaud upon auscultation of the head, and by the equally negative observations of MM. Fournet and Vernois, in nine or ten cases of meningitis, and of Professor Piorry,[1] we thought it right at that time, and in subsequent editions of our work, to express doubts of this new application of the stethoscope, and were led to dispute, if not the reality, at least the importance of auscultation of the brain.

Still I always regretted this summary condemnation of the undoubtedly conscientious labours of two professional brethren. That results stated in America as certain should be absolutely and generally denied in Europe seemed not a little extraordinary; and we must either suspect the mode in which the physicians on the other side the Atlantic made their observations, or confess that in this hemisphere we were deaf to sounds distinctly heard in the other. Shall we say with Harvey's adversary, the old Venetian physician, "*quem nos surdastri audire non possumus*"—"in Europe people are deaf; they only hear that in America '*tantum modo in America exauditur.*' "

As the only means of deciding the question was by making more minute clinical observations, I devoted myself patiently to this difficult task, and began afresh the study of cerebral auscultation. My researches were almost exclusively among children,[2] because it is chiefly among them, when at the breast, that Drs. Fisher and Whitney said they heard the stethoscopic phenomena, the discovery of which they announced; and because the difficulty of diagnosing diseases of the brain is greater in childhood than in later life, a greater interest belongs to the progress which symptomatology would make in this department. I may add in passing that it is only among children, whether subject or not to cerebral disease, that auscultation of the brain is likely to be of use; for beyond a certain age, or beyond a certain period of early childhood, the most practised ear applied to the cranium cannot detect morbid cephalic bruits.

Since I began collecting the observations which form the subject of this memoir, several authors have been similarly engaged in cerebral auscultation, among others MM. Rilliet and Barthez, and M. Hennig, head of the Children's Hospital at Leipsic. The first, in their admirable work (2d edit., vol. ii. pp. 158–9), make some remarks on the cephalic souffle in cases of hydrocephalus and in rachitis; I shall have occasion to quote the passage where they speak of this bruit under the head of the diagnosis of these affections. The German author has written a monograph upon this subject, and in a pamphlet of not less than thirty pages (*Archiv für physiologische Heilkunde.* Stuttgart, Août 1856) he reproduces, with annotations and additions, the inaugural thesis of Dr. Wirthgen (*De strepitu qui in capite auscultando auditur*, Leipsic, 1855), which, indeed, appears to owe its birth to him, and in which he successively studies the normal and abnormal bruits discovered by cerebral auscultation, the seat of those bruits, the age at which they are heard, their physiology and pathology, and especially the mechanism of their formation, and from these he deduces the results which the physician may expect to find in practice. I had completely devoted myself to this study before I became acquainted with the researches of M. Hennig, and we shall see, by the details of my observations, how far my conclusions agree with his.

Perhaps the admirable work of the Leipsic physician is faulty from a want of method and perspicuity, of which French readers complain greatly in the writings of the learned German author. M. Hennig does not give with sufficient clearness the details from which he drew his deductions, nor his mode of proceeding in collecting facts; the number of cases from which he decides (and they do not appear to be very numerous) are not indicated with precision; all this necessarily diminishes the value of his statements.

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[1] Traité de pathologie iatrique, tom. viii. p. 353.

[2] I have never been able to detect any cephalic bruit among adults, either in apoplectic patients or young chloro-anæmic females.

In order that I may not incur the reproach just cast upon the German author, I may say that the conclusions which are recorded in this paper were drawn from an examination and comparison of a very considerable number of observations, their total being nearly 300; at first they were gathered from cases selected among children whom I thought were affected with cerebral disease, or who were the subjects of rickets; then (having determined the existence of a cephalic souffle among children of healthy appearance) I afterwards examined all cases indiscriminately, whether in the wards of the Hospice des Nouveaux Nés, the Hôpital des Enfants, or in private practice.

Plan of the Memoir.—Having made these preliminary observations, we come now to the study of facts, and after having traced out the *rules* which facilitate auscultation of the head, I shall point out what *physiological bruits* the ear discovers when applied to the cranium; and in a third chapter, the longest and most important, *morbid bruits* will be specially considered.

SECTION I. *Rules for Cerebral Auscultation.*—When a patient is submitted to stethoscopic examination the most convenient position is the horizontal, with the head placed upon a pillow slightly raised; this may be inclined to different sides, according as it is desirable to examine the occipital, frontal, or temporal regions, &c.

In examining an infant at the breast, it should be seated on the knee of its mother or nurse. and the head should, if possible, be held steadily, either by the hands placed softly on either side, or by leaning it against the mother's breast. If the patient be in a comatose condition from some cerebral affection, auscultation will be easily performed. It becomes difficult when the child cries or moves about, which is usually the case; we must then try to quiet it by suckling and auscult it while it is at the breast, the bruit of deglutition being no obstacle to the recognition of morbid cephalic bruits.

If the examination be impossible while awake, it must be abandoned till an attempt can be made in quieter moments, or we may make the attempt during sleep, which is generally sufficiently profound not to be disturbed by the examination of a physician.

The auscultation may be either *direct* or *indirect*. Dr. Fisher found the latter the most convenient, the ear adapting itself exactly to the rounded or projecting surface of the cranium. Care must be taken to prevent the rustling of the hair; also, as a matter of cleanliness, the child's head should be previously covered with a napkin, which may serve at the same time to keep it more firm. Auscultation, practised thus directly, possesses some advantages; whatever may be the cries or agitation of the little patient, one always finds, in applying the ear quickly and somewhat firmly to the middle of the anterior fontanelle, that we catch, so to say, the sound of a cephalic souffle. But when the child is asleep, or when it is willing to remain quiet, mediate auscultation is preferable; it allows the detection of the bruits with greater precision and exactness, and we may also isolate them, so that those which are natural are not confounded with those which are abnormal.

For this purpose the ordinary stethoscope, which has been generally substituted for the primitive cylinder of Laennec,[1] answers perfectly; held as a pen in writing, it should be applied over the different regions of the cranium, and be kept fixed in the same way as for auscultations of the vessels of the neck. M. Hennig recommends the employment of a flexible stethoscope, twenty-three centimetres long, having for its middle portion a tube of vulcanized India-rubber. This stethoscope is certainly preferable, for with it we can easily follow the movements of the child during the examination.

On what regions of the head should the instrument be applied in order the better to detect the physical phenomena? If Drs. Fisher and Whitney are correct, the bruit of the cephalic souffle is so evident that it suffices to apply the ear to any part of the cranium in order to discover it immediately; but our experience does not bear out this assertion. It may be true that the bruits of nasal respiration, and of deglutition, are perceptible over nearly the whole surface of the head, but this is certainly not the case with the genuine cephalic souffle, which we have most frequently heard in the region of the anterior fontanelle, and still more plainly when we auscult directly over the fontanelle itself. It gradually diminishes in intensity the further we remove from that point, which appears to be the centre whence the bruit starts; then gradually becoming more feeble along the parietal or frontal sutures, it is completely lost further away, the finest ear failing to discover any sound over the lateral surfaces of the cranium and the occipital protuberances.

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[1] Traité Pratique d'Auscultation, cinquième edition, p. 19.

The physician who cannot count for long on the docility of his little patient would do well, if he wishes to detect the cephalic bruit, at once to place his stethoscope on the summit of the cranium over the depression corresponding to the anterior fontanelle; it is there, as we have said, that the abnormal bruit exists in its greatest intensity, and the examiner, if he does not detect it at that point, may dispense with any further investigation.

SECTION II. *Physiological Bruits.*—When we practice auscultation on the top of the head of a healthy subject, different bruits are heard.[1]

I. One is produced by the circulation of the air in the nasal fossæ: this is the *cephalic bruit of respiration* mentioned by Dr. Fisher; it is very intense and harsh, and analogous to the laryngeal respiratory murmur; it coincides with the respiratory movements, increasing in strength and frequency when these are accelerated.

These characters are sufficient to distinguish it from other cephalic bruits; but in some cases of extreme dyspnœa the nasal respiratory bruit is so frequent and quick that it might easily be mistaken for the cerebral vascular souffle of which we have just spoken, and the two phenomena might be confounded were it not for the close relation which the first bears to the movements of the thorax.

When the pituitary membrane is thickened or there is much secretion of mucus, the respiratory cephalic bruit is transformed into a sibilant rhonchus, or into a kind of coarse crepitation; the same physical conditions of the mucous membrane exist as in bronchitis, and consequently the same râles are heard.

Nor is this all; in affections of the larynx, the bruits caused by the passage of the air along the laryngo-tracheal tube are much drier and more or less prolonged, extending above into the nasal fossæ as well as into the bronchial tubes below, and the rhonchus or laryngeal hissing (similar to the bronchial râles) everywhere heard is more perceptible in auscultation of the head than it is in that of the chest.

With regard to the râles formed in the lungs, these are not heard from so great a distance, and they escape stethoscopic examination when practised on the head: in the very exceptional case of a young phthisical girl I was able to detect, by cerebral auscultation, a loud gurgling which was produced in a cavity in the subclavicular region.

II. If the child speaks or cries the *vocal sounds* are heard very distinctly, and the ear, when applied to different parts of the cranium, perceives a very remarkable resonance; this *cephalic resonance* may be compared to that which is heard so loudly in the larynx, it has a nasal timbre which varies in different individuals, it is always very piercing, the voice seeming to proceed from the cranium.

III. Placing the stethoscope on the head, another peculiar bruit may be hea that of deglutition; in new-born infants there is also that of suction. This bruit is so singular and so readily recognized that there is no need to describe it. We hear it more distinctly by direct than indirect auscultation, and in cases where the nurse appears to have but little milk, and when consequently the child makes frequent efforts at suction without our being able to decide the fact of its having swallowed anything, we may be able to determine this by cerebral auscultation, the bruit of deglutition comes very strongly upon the ear, and we are therefore better able to appreciate the quantity of milk supplied and swallowed.

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[1] In treating here of *auscultation of the head*, I shall not consider *aneurisms* of the larger arteries of the brain nor *diseases of the ear* (see the work of M. Gendrin and an admirable article by M. Ménièr, in the *Traité pratique d'Auscultation*, 5th edit. p. 562.)

Such are the *extrinsic bruits* revealed by cerebral auscultation; moreover, it is not impossible to a practised ear to hear in some subjects, especially in cases of palpitation, the two sounds of the heart propagated from the solid walls of the cardiac region up to the head. In forty-two healthy infants five times I was able with attention and patience to perceive the tic-tac of the heart in ausculting over the cranium. In another case, I have been able to hear a strong bellows-sound originating in the left auriculo-ventricular orifice; but this last case was exceptional. More frequently the cardiac sounds themselves escape observation in auscultation of the brain; and we may, for greater simplicity in studying the subject, omit the *cardiac cephalic bruit*, at least as it has been described by Drs. Fisher and Whitney.

With much greater reason we shall contest the possibility of recognizing with the ear the modifications which so-called cerebral diseases create: for example, Dr. Fisher says he is able to discover in almost every case (thanks to his aptitude for cerebral auscultation), the impulsive character of the pulsation which he supposes to belong to apoplexy. These, however, are minutiæ of such exceptional cases (*rara non sunt artis*), and tend only to embarrass without enriching semeiology.

IV. Are we, then, to include in the physiology of this subject an *intrinsic bruit*, the *cephalic souffle*, an essentially pathological phenomenon?

On this point, those authors who have specially directed their attention to auscultation of the brain are far from being agreed. With the American observers, the souffle is always abnormal, it is never a healthy phenomenon. Dr. Fisher expressly says, "it depends always upon some cerebral affection." Dr. Whitney equally insists upon the proposition, that auscultation never reveals anything like a cephalic souffle in the normal condition.

M. Hennig, on the other hand, believes that the souffle belongs rather to a state of health;[1] and, further, this bruit, which according to the American doctors would be much more manifest as the cerebral affection became more marked, would, according to the Leipsic physician, sometimes disappear in sick children, to reappear with returning health.

If we seek among our own facts whether the American or German authors are right, the first declaring that in the normal state the cephalic souffle is never heard, and the second being of opinion that it is always heard; we incline to believe that both are equally wrong, and that these two exclusive opinions are alike to be condemned.—*London Med. Rev.*, July, 1862.

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19. *On the Transmission of Syphilis by Vaccination.*—The very interesting remarks which follow are from a clinical lecture delivered by M. Ricord, at the request of M. Trousseau, in the Hôtel Dieu, at Paris. They start from a case then in the hospital: they end with an opinion upon the notorious case at Rivolta, in which syphilis broke out in forty-six infants three weeks after vaccination.

A young woman, aged eighteen, was admitted on the 6th of December last into the wards of the Hôtel Dieu, for the treatment of catarrhal metritis and granulating ulceration of the cervix. *No syphilitic precedent whatever* could be traced in her history. An epidemic of smallpox having broken out while she was in the hospital, all the patients liable to contagion were repeatedly vaccinated. Although the girl presented unmistakable marks of previous successful vaccination, she also underwent the operation. The lymph was supplied by a healthy infant, born in the wards, and vaccinated a few days before with the virus distributed at the Academy of Medicine; three punctures were made in each arm. Three children were also inoculated with the matter taken from the same infant, and in all four the pustules were developed in the most regular manner. In the young woman, on the contrary, the vaccine did not take; this was fully expected. She left the hospital, and was lost sight of for a fortnight, when she returned to the Hôtel Dieu, complaining of pain in one arm. On examination of the part two pustules of ecthyma were found occupying the seat of two punctures. These were at first referred to a somewhat tardy evolution of the vaccination, and perhaps to accidental friction. But the pustules, deemed insignificant at first, gradually became larger and hard at the base; cervical and axillary adenitis set in, and, after an interval of five or six weeks, a roseate eruption broke out over the entire body.

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[1] So we gather from several rather confused passages in his Memoir where cephalic bruits are considered. "We have heard these bruits (M. H. does not say which) on the head from the twentieth week to the sixth year. . . . These bruits have been present in healthy children at the twenty-third week." (*Loco cit.* p. 413.) "The souffle or hissing which is present in *healthy* children." (*Ibid.* p. 415.) "These bruits are sometimes wanting in sick children, but reappear with convalescence." (*Ibid.* p. 413.)

From the beginning, M. Trousseau strongly suspected the syphilitic nature of the symptoms, but he was desirous of obtaining the opinion of M. Ricord, who fully confirmed his conjecture.

It is an unquestionable fact, said this last-named physician, that this patient bears on her arm a most distinct primary sore, *an indurated and infecting chancre*, characterized by an indolent, convex tumour, ulcerated at the point (*ulcus elevatum*), suppurating moderately, and supported on a broad, elastic base, well limited at its margin, surrounded by uninflamed textures, and, as it were, implanted in the healthy tissues. The present tumour is a fair specimen of the infecting chancre in a state of transition towards the secondary stage, and assuming the aspect of the mucous papula. Consider, in addition, the other phenomena which have followed each other in rapid and regular succession, the glandular enlargements of the axilla and neck, the headache and roseola, and you cannot doubt but that the case before you is an unmistakable instance of genuine secondary syphilis.

With regard to the origin of the disease, it is obvious that the punctures in the arm have been the portals through which it has entered the system; but it is by no means so clear that the poison was introduced into these wounds together with the vaccine-lymph.

M. Ricord does not reject this mode of propagation as absolutely impossible. But in the estimation of facts which seem to establish such transmission, it is necessary, said he, to distrust the evidence of our senses. It is only by taking into account the obscurity which must necessarily surround a pathogenic interpretation of an usually retrospective character, and carefully guarding against the errors that a superficial observation may give rise to, that we can hope to discover the solution of so important and intricate a problem. We have now entered, he continued, upon a period of reaction against hitherto accepted doctrines, and if we do not take care, we will be almost inclined to pronounce a man to be affected with syphilis if he has ventured, without an umbrella, in some of the less reputable streets of this capital. M. Ricord then treated of the contagiousness of constitutional syphilis, and ascribed it in most instances to the infectious character retained by chancres, *undergoing transformation* into secondary symptoms, or by *mucous papulæ*, the earliest manifestation of the general poisoning of the system, whether developed *in situ* by a metamorphic change of the primary sore, or at a distance from the seat occupied by the latter. However this may be, M. Ricord contends that the instances of propagation of syphilis by the contagion of secondary symptoms, are far more unfrequent than some authors have asserted. Syphilis is widely diffused, and were the contagion of its constitutional manifestations as easy as has been affirmed, the nineteenth century would far outrival the fifteenth.

There may be perhaps some other vehicles of contagion besides the secretion of chancre, and sometimes of a secondary sore; this at least is not impossible, but is not susceptible of peremptory demonstration in the present state of our knowledge. An erroneous and hasty interpretation of obscure facts, in which the true filiation of the symptoms has eluded detection, is, in M. Ricord's opinion, at the bottom of all the mistaken theories and wandering speculations propounded on the subject. Thus, some men may escape infection though they have communicated with a woman affected with chancre, and others may be poisoned by one who herself is sound. Such cases are, of course, not obviously intelligible at first, but the obscurity is easily cleared away by the now well-known history of *mediate contagion*, by which we are taught that healthy females, who after intercourse with diseased men are approached by persons in sound condition, may transmit from the former to the latter the virus, themselves escaping scatheless. M. Ricord here related several very curious instances which illustrate and confirm in the most distinct manner the doctrine so perspicuously laid down by M. Cullerier. On the other hand, it is unnecessary to expatiate on the innumerable modes of conveyance of the virus from one person to another. Every possible contact, every imaginable form of communication, may generate chancre. M. Ricord once attended a magistrate affected with indurated chancre of the eyelid, periauricular bubo, and secondary roseola. The organs of generation were perfectly sound, but the patient acknowledged that his hands had wandered into dangerous precincts, and that in rubbing his eyes he had inoculated the virus into the eyelid. Is it entirely impossible that something of the same kind may have happened in M. Trousseau's patient? She was lost sight of for a fortnight; the punctures in the arm induced some local uneasiness, which she doubtless endeavoured to allay by scratching with her hand. Now, who will say that that hand has constantly remained pure, or that the arm may not have been exposed to any other suspicious contact? These are some of the points which ought to be cleared up before vaccination can, with any show of reason, be assumed to have caused the chancres observed in this instance.

The possible transmission of syphilis by the inoculation of the blood of persons tainted with the virus has, however, been for some time before the public, and derives considerable additional importance from the results recorded by M. Viennois. In this memoir the author agrees with M. Rollet, that syphilis is not transmitted by vaccine-lymph, but by the admixture of the latter with blood. In a recent vaccination, which caused considerable sensation beyond the Alps, it has likewise been contended, in order to account for the propagation of syphilis, that blood oozed out, together with lymph, from the pustules of the child who supplied the matter, and that the lancet of the operator was therefore charged with a mixed fluid of deleterious nature.

M. Ricord, while admitting the truth of these facts, rejects the interpretation which has been offered.

It is a remarkable circumstance, said he, that as soon as the generating poison of syphilis has penetrated into the system, it is fundamentally modified. It becomes undiscoverable to chemical analysis, or to microscopic research, and utterly loses its distinguishing character of reproducing a pustule similar to that in which it was originally generated. Were it otherwise, in a person tainted with syphilis, the most trifling wound would be liable to assume the aspect of chancre, from contact with the blood escaping from the lacerated vessels. Nothing of the kind is ever observed. M. Ricord has performed operations on many individuals suffering from constitutional syphilis, and he never noticed, even in a single instance, anything particular in the aspect, progress, or duration of the wounds.

It will, perhaps, be alleged, that experiments have been instituted, which seem to point to the possibility of the transmission of syphilis by the blood of diseased subjects. Thus, M. Waller, after scarifying the thigh of a patient, dressed the wounds with lint impregnated with syphilitic blood; the part healed, but about three weeks afterwards pustules appeared on the thigh. The fact is true, says M. Ricord; but it is proper also to add, that *at the same time* a similar pustule was developed on the shoulder. This chancre had, doubtless, the same origin as the others, and entirely invalidates the case. At Lyons, analogous experiments yielded conflicting results. Finally, M. Lalagade, the head surgeon of the Hospital of Albi, who never had been affected with syphilis, *publicly* inoculated in his own arm the blood of three soldiers manifestly suffering from this complaint; and although, on each occasion, two wide and deep punctures were performed, the results were entirely negative.

As to those sad instances of vaccination published in Germany, in France, and in Italy, M. Ricord does not deny their accuracy; they assuredly deserve consideration, but cannot be received as conclusive in the question, until all their attendant circumstances have been minutely weighed, and the reciprocal operation of cause and effect has been carefully inquired into. M. Ricord declared that he gave his full approbation to the conclusions of a paper, published in the *Gazette Hebdomadaire*, by M. Jaccoud, on the distressing occurrences observed during the course of the last year in the province of Acqui.

M. Jaccoud, after reminding his readers that at the end of the month of May, 1861, syphilis broke out, three weeks after vaccination, in forty-six infants at Rivalta, proceeds to state that an inquest was held as to the cause of the calamity; and that, despite the apparent clearness of the facts, the committee appointed to investigate the matter declined to pronounce on the alleged connection between syphilis and vaccination, and declared that, in order to form their judgment, further inquiry was necessary. He continues thus:—

"These gentlemen were both prudent and wise. Before admitting that syphilis in this instance was transmitted with the vaccine matter, many difficulties and obscurities have to be cleared away; it would further be necessary to solve several important questions, which at present it appears impossible to reply to. For our own part, an attentive perusal of the documents of the case has led us to the same conclusion as Dr. Albertetti, who exonerates from all blame the vaccinations in question. The events of Rivalta present to our view two conspicuous, but wholly distinct facts—viz., the vaccination of the infants, and the subsequent appearance of syphilis in certain of their number. It is granted that these two orders of facts occurred in succession, but for the present we are not prepared to go any further, and to argue, *post hoc ergo propter hoc;* the coincidence is obvious, not so the inference of causality. Whatever interpretation be adopted as to these facts, they convey a useful caution, and illustrate in an eloquent manner the necessity of taking into account the manifold elements in the ætiological history of vaccinal syphilis, and the extreme reserve required of the physician in cases of this description."

"This view," said M. Ricord, "is in such perfect harmony with mine, that I have nothing to add to M. Jaccoud's remarks. Let us admit, and carefully inquire into, these cases, and let us guard against any predetermined notions on the subject; but as to the interpretation offered, let it be received with an amount of hesitation and doubt, increased by the obvious fact, that if ever the transmission of syphilis with vaccine-lymph is clearly demonstrated, vaccination must be altogether discontinued, for, in the present state of science, we are in possession of no criterion which may permit the conscientious practitioner to assert that the lymph he inoculates is perfectly free from admixture with blood tainted by syphilis."—*Ranking and Radcliffe's Half-Yearly Abstract*, vol. xxxv., 1862.

20. *Treatment of Agues by Cinchoniæ Disulphas.*—Mr. H. R. OSWALD states (*Madras Quarterly Journ. Med. Sci.*, Oct. 1861), that he has been prescribing the cinchoniæ disulphus in the Shemooga Hospital since 1857, up to the present date; during this period 4,985 cases of fever, of all types, have been treated, so that he has had numerous opportunities for testing its virtues, and his experience has led him to form a high opinion of its powers as an antiperiodic. Intermittent fever, as seen there, though not often fatal, is in many cases very severe; the ague especially is prolonged, and enlargement of the spleen frequently met with, even in children of the tenderest years; showing the truly malarious origin of the disease. "With this field for observation," says Dr. O., "I may state that I am inclined to place cinchonine almost on a par with quinine; in some cases it fails, but not often. Doctor Kirkpatrick observes, the dose required to check an attack of ague is about the same as that of quinine, but I do not quite coincide with this, as I have generally found a rather larger quantity necessary.

"I have not tried the combination of quinine and cinchoniæ disulphas, as in cases where the latter has failed I have at once had recourse to the former.

"Royle states that 'Dr. Pereira and others consider it equal in medicinal activity to the quinæ disulphas.' Dr. Waring says, 'its action is similar to that of quinine, but it is less energetic, and consequently requires to be given in larger doses.' Many cases in which it has been given might be adduced, but, as they would occupy valuable space, it may be sufficient perhaps to mention, that since its first employment by me, no less than 4 lbs. 10 oz., have been expended in addition to quinine and bark, showing the estimation in which it is held. In fact it is now in daily and constant use, and is as regularly indented for as any other drug.

"For some time past, however, I have observed that the administration of the cinchoniæ disulphas is attended with certain physiological effects, which will probably prevent it from ever being substituted for quinine under all circumstances. Dr. Kirkpatrick has mentioned that nausea and vomiting attend the use of large doses, such as 15, 20 and 30 grains. I have, however, found that nausea sometimes attends even 5 grain doses, and this is frequently accompanied by giddiness, dryness, and constriction of the fauces, and sometimes purging with pain in the abdomen. I observed some of these symptoms in my own person after a 10 grain dose taken to check an attack of fever, and the same remark has been volunteered to me by natives who have taken it: they pass off in a few hours; as long as they last, however, they are very disagreeable, as I can testify from personal experience.

"After a time there is apparently a 'tolerance' of the remedy, and these unpleasant symptoms are not perceived; in long-continued doses the effect is similar to that produced by quinine.

"I think then with these disadvantages the remedy is never likely to supersede quinine, except where economy is a consideration. When this is the case, I have no doubt that the cinchoniæ disulphas may be substituted for quinine in the treatment of simple uncomplicated intermittents with equal safety and benefit."

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21. *On the Treatment of Pneumonia: with the Results of One Hundred and Five carefully recorded Cases.*—At the late meeting of the British Medical Association in London Dr. J. Hughes Bennett, of Edinburgh, read an interesting paper under the above title.

By pneumonia, or inflammation of the lung, the author stated he understood a disorder essentially composed of an exudation from the blood among the elementary textures and into the air-vesicles of that organ, which gives rise to those well-known physical signs and functional symptoms with which all are familiar. This, like most other inflammations, when acute, was formerly treated by so-called antiphlogistics—that is to say, bloodletting, purgatives, antimonials, low diet, and other methods of lowering the strength of the patient. It was about eighteen years ago, in consequence of investigating the pathology of inflammation, that Dr. Bennett began to doubt the propriety of such a treatment, and this for the following reasons:—

In the first place, the cause of the inflammation is an irritation of the textures, of the ultimate molecules, of the part; in consequence of which their vital power of selection is destroyed and that of their attraction is increased. The removal of blood by venesection cannot alter this state of matters, neither can other lowering remedies. If the inflammation be superficial and limited, local bleeding may diminish the congestion, as in conjunctivitis; but if exudation has occurred, it cannot remove that.

In the second place, an exudation or true inflammation having occurred, it can only be absorbed by undergoing cell-transformation. Now this demands vital force or strength, and is arrested by weakness. Inflammations in healthy men rapidly go through their natural progress. In weak persons they are delayed or arrested: hence their fatality.

In the third place, the strong pulse, fever, and increased flow of blood in the neighbourhood of inflamed parts, have been wrongly interpreted by practitioners. They are the results, and not the causes, of inflammation; and show that the economy is actively at work repairing the injury. So far, therefore, from being interfered with and interrupted, they should be supported by nutrients.

It follows, fourthly, that if these views be correct, the true treatment of inflammation should be directed towards bringing the disease to a favourable conclusion by supporting, rather than by diminishing, the vital strength of the economy; and this, not by over-stimulation, as was done by Dr. Todd, but simply by attending to all those circumstances which restore the nutritive processes to a healthy condition.

Having been guided by these views in his practice for the last fourteen years, and having seen that they have been gradually adopted by the profession, Dr. Bennett offered the most convincing proof of their correctness from an analysis of 105 cases of pneumonia publicly treated in his clinical wards in the Royal Infirmary of Edinburgh, and carefully recorded by his various clinical clerks. In all these cases the treatment was directed to the support of the economy, never to weaken it by antiphlogistics. At the same time, if dyspnœa be urgent, cupping or a small bleeding (from four to eight ounces) may be practised as a palliative, more especially in bronchial or cardiac complication—although in none of these cases was such bleeding ever found necessary by him. During the febrile excitement mild salines are administered. On the fourth or fifth days, when the fever abates, good beef-tea and nutrients are administered; and on the pulse becoming soft or weak, from four to eight ounces of wine daily. As the period of crisis approaches, slight diuretics are given to favour the excretory process. The results are—

Single uncomplicated cases . .	58	Average duration	13½ days.
Double " " . .	19	" "	20 "
Complicated cases	17	" "	15⅓ "
Unsatisfactory cases (as to duration)	8		
Deaths	3		
	105		

Ratio of deaths, 1 in 35 cases.

Average residence in the hospital of 77 uncomplicated cases of pneumonia (single and double), 22⅛ days.

It has been supposed that in consequence of this comparatively small number of cases occurring in so long a period as fourteen years the disease is rare in Edinburgh. It should therefore be explained that the clinical professors are on duty alternately, and that he (Dr. Bennett) had never acted as physician to the Infirmary more than one-half of the year, and in most cases only one-third of the year. It has also been imagined that pneumonia in Edinburgh is unusually slight and trivial, or that the disease in these cases was not extensive. But it is not so. Many cases, and especially the double ones, were very severe, with great dyspnœa and very urgent symptoms. Dr. Bennett had frequently pointed out the existence in these cases of the hard and strong pulse in vigorous young men, in whom, however, the most rapid recoveries were invariably observed. It should also be noted that these cases were in no way selected, but included not a few which were admitted *in extremis* by the resident clerk, and never seen by the physician; nor such as were partly treated by other physicians in the hospital, and for which treatment Dr. Bennett was not responsible.

From these facts Dr. Bennett concluded:—

1. That simple pneumonia, if treated so as to support, instead of lower, the nutritive processes, so far from being a fatal disease, almost invariably recovers.
2. That the cause of mortality in these cases is exhaustion, either before they come under medical supervision, or, as formerly practised, by a lowering treatment. Bleedings or other remedies that do not exhaust must be regarded as palliative rather than curative, and their influence has yet to be determined with exactitude.
3. That the same rule applies to all inflammations, the amount of danger being in direct ratio to the weakness of the system; the existence of complications in other viscera, or blood poisoning.

Dr. Bennett was of opinion that these important results were not the effect of chance; of empirical experiment; of a change in the nature of inflammation, or of the force of the pulse in man and animals; of an alteration of diet or of drink, or of nervous susceptibility; nor of a change of type in disease: all of which have been supposed by some explanatory of facts which can no longer be denied. The more he considered this subject, the more was he convinced that it could only be justly attributed to the advance of medical science, and that it was a source of infinite satisfaction for medical men so to consider it. He thought it strange that some minds would rather ascribe so manifest an improvement in the treatment of disease to hypothetical revolutions in nature which had no proof in their support, than to the increase of knowledge amongst the profession which was obvious to all. It seemed to him that if any one demanded in what way our advance in physiology and pathology had benefited the treatment of disease, no better proof of it could be found than in the diminished mortality that everywhere now accompanied attacks of acute inflammation.—*Lancet*, August 16, 1862.

22. *The Therapeutics of Delirium Tremens.—The Madras Quarterly Journal of Med. Science* for October, 1861, contains some interesting observations on this subject by J. L. Ranking, Esq., Surgeon Major, H. M.'s 105th Regiment L. I.

"Within the last few weeks," he remarks, "the payment of the 'bounty' has afforded the men the means of indulging their propensities for drink to an extent which probably never fell to their lot before. Some thousands of rupees were put into circulation, and the consequence was that 81 cases of ebriositas (drunkenness in its most profound form), and 14 cases of delirium tremens passed through the hospital.

"The experience thus afforded us has led to our forming an opinion as to the therapeutics of delirium tremens which we may express in the following propositions:—

"1st. That in *the very large majority of cases alcohol* may not only be *safely* but *advantageously dispensed with.*

"2d. That *it, or other accustomed stimulus, cannot be entirely laid aside in old soldiers;* men who have drank hard all their lives, and who have experienced prior attacks of the disease.

"3d. That *opium* is *unnecessary* in *all cases, and positively injurious in many.*

"Or to express the propositions differently:—

"1st. The *young* soldier, *whose eliminative powers are unimpaired*, can be treated with safety and advantage without a drop of alcohol or a grain of opium.

"2d. The *old* soldier *with organic visceral disease*, requires no opium; but *does require some of his 'accustomed stimulus,'* combined with eliminants and nutrients.

"3d. That *opium is unnecessary in all cases;* that it often fails to induce sleep, which succeeds to its withdrawal.

"4th. That if pushed to its full extent, it may and sometimes does induce a sleep passing into coma and death."

The result of the treatment of 14 cases upon the plan indicated by Mr. R. are very encouraging:—

"Of these 14 cases, three presented all the symptoms characteristic of the fully developed disease, or of the third stage. In two there were unquestionable evidences of organic disease of the kidneys; a complication of a most formidable nature, and one materially influencing the result. One, treated *entirely without opium* and *alcohol, died;* the other, *who had a moderate allowance of alcohol, recovered.* The third, a young soldier but with a very severe attack of the disease, recovered without, as before said, a grain of opium or a drop of spirit.

"The mortality in relation to these 14 cases, then, stands as follows: One in 14 as relates to the whole group without reference to the *stages* of the disease.

"Or, all in the first and second stages of the disease recovered, while of those advanced to the third stage one in three died.

"Without appearing to contend too warmly for the treatment without opium and alcohol, we may advantageously inquire, would the case lost, taking into consideration the condition of the kidneys, and we may add also of the liver, which was far advanced in fatty degeneration—would that case have benefited by the exhibition of opium and alcohol, one or both? Knowing as we do, that opium is very ill borne in all cases of organic renal disease, that a dose of that drug which may safely be given to a healthy individual, may prove a poisonous dose to one whose kidneys are organically diseased, our answer must be, we think, in the negative. Alcohol, in a similar case occurring to us, we should with the experience since afforded us, certainly give in *moderate* doses, to sustain the vital powers while the blood was being rid of its urea; for in the case alluded to, we have no doubt that death resulted from uræmic poisoning; the condition of the urine during life, and examination of the kidneys after death, proved that they were spoiled for all the purposes of elimination.

"We would also remark that this case is the only one which has occurred in our practice, which in the least degree favours the views of those who consider that the disease is often caused by the withdrawal of the accustomed stimulus.

"The patient was a known sot. On admission his case did not appear a very severe one, and it was not till the third day after admission that serious symptoms arose. Would a pretty liberal allowance of brandy from the commencement have saved him?

"It remains for us to give the promised instances of the injurious effects of large and repeated doses of opium.

"In two cases in which many nights had been passed without sleep, cases which we had treated on the eliminant and nutritive system entirely, it appeared desirable to force sleep. In each case ʒiss of the liq. opii sedativus was prescribed at 8 P.M., and ʒss directed to be repeated every two hours till the patient slept. In both instances we were summoned after midnight, the patients being in a state of alarming sinking or collapse, both under the impression that they were dying. In both instances the opiate was withdrawn, and camphor and brandy given. They rallied slowly, and both slept the following night after the exhibition of a glass of hot 'brandy and water.' We could not attribute the condition of collapse in these cases to anything than the liq. opii—the patients themselves attributed their feelings to the opiate, and both rallied under stimulants after the withdrawal of the opiate. The *ill* effects of the opiate and the *beneficial* effects of the brandy were to our mind most apparent. These were the only two cases in which full doses of opium 'fearlessly repeated' were tried—no sleep followed, although in each case ʒiiss of the drug had been taken, and our firm impression and belief is, that, if we had continued it till the patients slept, they would have slept the sleep of death.

"We are aware that we have advanced nothing new in this paper—all has been said before, much better than we have written it.

"We are, however, so strongly impressed with the weight of the moral and, we may add, economic grounds upon which the system advocated is based, that we have considered it our duty to show the extent to which we have been able to apply it during a period of an almost unprecedented excess.

"The economic value of the system as applied to this regiment we will elucidate in our concluding remarks.

"Since the arrival of the regiment at this station in April, 1858, up to the date of our assuming charge, we find 171 cases of 'ebrietas' and 'delirium tremens,' with a total expenditure of brandy (*in such cases only*) of bottles 96 and measures 5, or allowing 24 ounces to each bottle of 14 imperial gallons.

"In our practice between 81 cases of ebriositas and 14 of delirium tremens—total 95—the expenditure has only been bottles 3 and measures 5½.

"On the old system it would have equalled (taking the average of 171 cases above given) some 53 bottles, represented in value, at the commissariat rates of Rs. 18-4-0 per dozen, by Rs. 84.

"We hope, or we should rather write *expect*, to have further opportunities afforded us of testing the merits of the system of treatment without alcohol and opium. We do not think, however, that further experience will materially modify the opinion we have already expressed.

"The systematic examination of the urine for evidences of organic renal disease should never be omitted. Prognosis hinges mainly upon the existence or non-existence of organic changes in the kidneys and liver, and we have a strong suspicion that the large majority of cases which terminate fatally will be found to be associated with such pathological conditions, as forbid the hope of elimination of the poison of alcohol, at the same time that they offer the further complication of uræmic poisoning."

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23. *Delirium Tremens—Large Doses of Tincture of Digitalis—Cure.*—M. F. **Manifold** relates (*Med. Times and Gaz.*, Aug. 23, 1862) the case of a soldier,

aged 27, stout and muscular, brought to the hospital January 4, 1861, in a state of great nervous depression and mental anxiety after a series of debauches during Christmas week, ending in a convulsive fit, in which condition he was conveyed to the hospital from his barrack-room. He was immediately placed in bed, in a ward by himself, in charge of an European orderly, and directed the following draught—℞. Carb. ammoniæ gr. v, mist. camphoræ ℥j, liquor opii sed. ♏xx; fiat haustus s.s. et rep. tertiis horis. He was also ordered beef-tea, and the head to be kept cool.

January 5. Has had no sleep. Tremens well marked. Endeavours to get out of bed, and is with difficulty prevented from doing so. To have a calomel purge, followed by a cathartic draught combined with antimony, and one grain of morphia at bed-time; also, beef-tea and beer.

Third day. Very restless and delirious, talks wildly and incoherently, and has had no sleep since his admission into the hospital. Morphia combined with antimony repeated every third hour without producing the desired effect.

Fourth day. Much worse in every respect; wandering and delirious, although conscious when spoken to sharply. To have tinct. digitalis ʒiij immediately, and the following mixture—℞. Tinct. digitalis ℥j, tinct. lavand. co. ʒss, mist. camphoræ ad ℥viij; fiat haustus. Dose ℥j omni secund. horâ.

Fifth day. Much better this morning, after a refreshing sleep. To continue the mixture.

Seventh day. Convalescent. From this date he improved gradually, although weak, and was discharged to duty on January 18, 1861.

Remarks.—The beneficial effects of the digitalis were well marked in this case, the first dose acting as a powerful sedative, allaying irritation, and producing sleep. I would have no hesitation, in a similar case to the above, giving half ounce doses of the tincture, to be repeated, if requisite, every second or third hour; but this will seldom be found necessary.

In a case in which I was lately consulted by Dr. Barn, Royal Artillery, in which there was well-marked insomnolence, jactitation, and delirium, and where both morphia and hyoscyamus have been administered in vain in large doses the second dose of tincture of digitalis acted like a charm.

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24. *Action of Quinia upon Phthisis.*—With the view of testing, so far as practicable, the general therapeutical value of quinia in the treatment of consumption, Dr. Richard P. Cotton prescribed it for twenty-five patients, in various stages of that disease; avoiding, as in all his previous experiments, any selection of cases, and excluding only those unfitted by the existence either of acute symptoms or special complications. The dose consisted, according to circumstances, of one or two grains two or three times a-day; and was continued for periods varying from three to ten weeks. Notes were regularly taken by Dr. Harington, resident clinical assistant.

Ten of the patients were in the first, six in the second, and nine in the third stage of phthisis. Sixteen were males, and nine females. Their ages varied from twenty to fifty years.

During the administration of the quinia seven improved *greatly*, five improved *slightly*, and thirteen either did not improve at all or became worse. Of the twelve improved cases, seven were in the first stage, two in the second, and three in the third stage; and, of the thirteen cases in which the quinine seemed to be inoperative, three were in the first stage, and ten were the subjects of more or less advanced tubercular softening. Thus it would appear that whatever good may have resulted from the quinine, it was the most decided in the early stage of the disease.

In fourteen of the cases cod-liver oil was taken during at least a portion of the time. There was an increase of weight in ten out of the twenty-five patients; such increase occurring in five who had taken the oil, and in five who had not taken it, but being most marked in the former.

In four cases the quinia appeared to disagree, producing dyspepsia and loss of appetite. In six instances patients who had made little, if any, progress under the quinia by itself, were more or less benefited when steel was added to it. Two of these cases were remarkably good illustrations of the combined influence of quinia and iron; one was in an early and the other in an advanced condition of disease, but both left the hospital with every local and general symptom in abeyance, and their health fairly good, after taking for several weeks two grains of quinia twice a-day, and a tablespoonful of steel wine immediately after dinner.

From these facts, compared with previous observations on other remedies, the following are the conclusions at which I have arrived:—

1. That although quinia may be well adapted to certain cases in which there is an evident cachexia, it is greatly inferior, as a general remedy in phthisis, to some other tonics, whilst in a few instances it is unsuited to the disease.

2. That the combination of quinia and iron is sometimes very beneficial.—*Med. Times and Gaz.*, Aug. 30, 1862.

25. *Treatment of Acute Dysentery by the Nitrate of Silver.*—Dr. DUCLOS considers that dysentery always commences at the lowest part of the large intestine, and if such be the case, then he argues that the local medication of this part must arrest the disease and prevent its extension, or at least must modify its virulence even in the worst cases. Local treatment being, therefore, admissible in this disease, the beneficial effects will be in proportion to its employment at the earliest period. Dr. Duclos was induced to employ nitrate of silver as the remedial agent, from observing its well-known efficacy in purulent ophthalmia, aphthous eruptions in the mouth, and other such affections, and also from witnessing the practice of M. Trousseau, who employs this salt with great success, both by the mouth and in injections, in cases of simple enteritis in very young children. M. Duclos treats dysentery in the following manner: a slight laxative is first administered, such as a small dose of calomel or seidlitz powder, in order to unload the intestine of fecal matter, and then a simple mucilaginous enema is thrown into the rectum. When the laxative and the injection are both evacuated, an enema is administered, consisting of twenty-five centigrammes of nitrate of silver (a centigramme is .1543 of a Troy grain) in one hundred and fifty grammes of water (a gramme is about fifteen grains), together with some laudanum if necessary, but the latter is only added if it is quite impossible to retain the enema for a few seconds. The enema is kept in the rectum for from four to ten minutes and then allowed to return, and as it has been administered immediately after an evacuation, it is certain that the silver salt has been in direct contact all the time with the diseased surface. An enema is thus given night and morning, and M. Duclos has even given three in twenty-four hours; and this treatment is continued for several days, with the double view of avoiding constipation, which is the habitual attendant of true dysentery, and of giving the enema immediately after a dysenteric evacuation has cleansed the last portion of the intestine. The results of this treatment are, that in general the stools are rapidly modified both in frequency and composition. Most frequently, from the period of the fourth or fifth enema, they become less frequent, and also less bloody and slimy; the tenesmus also diminishes with great rapidity, and the pain produced on pressure of the left side is less acute. No bad results were observed to follow this treatment, but sometimes an excessive irritability of the anus or the rectum rendered it necessary to change the form of administering the injection. The dose of the nitrate of silver was then reduced, or a greater or less quantity of laudanum was added to secure its tolerance by the intestine. Dr. Duclos has employed the nitrate of silver in the manner described in a great number of cases of dysentery, and often with great success, and he records five cases at length. His views have been confirmed by the observations of other practitioners, and they may be summed up as follows: 1. The nitrate of silver, administered at the commencement of acute dysentery, in appropriate doses, constitutes a remedial agent which has produced very excellent results in Dr. Duclos's hands; 2. Its action has been equally efficacious, whatever may have been the age of the patient, and in all the forms of dysentery; and 3. In certain cases, when administered quite at the beginning and always in injection, it has seemed to exert, at the end of a few days, an almost abortive action upon the disease.—*B. and F. Med.-Chir. Rev.*, July, 1862, from *Bull. Gén. de Thérap.*, August 15, 1861.

26. *Treatment of Acute Rheumatism, considered with regard to the liability to Affections of the Heart under different Remedies.*—Dr. Dickson read before the Royal Medical and Chirurgical Society (June 10) a paper on this subject, based upon a tabular condensation of the cases of acute rheumatism which were admitted into St. George's Hospital during the five years ending on December 31, 1861, and in whom the heart was, on admission, unaffected by the disease. The method of treatment adopted in any particular case depended very much upon the chance of the patient coming under one physician rather than another; and a comparison of the results would, to a certain extent, be a guide to the value of the means used. The cases, therefore, were classified according to the treatment made use of. As the main object of the investigation was to ascertain the effect of remedies in preventing cardiac mischief, the arrangement was not altered by measures adopted in consequence of its occurrence. The tables were fourteen in number. The first gave the results of eight cases in which venesection was early resorted to, other medicines being at the same time used. In three of the cases endocarditis or pericarditis was clearly recognized. In one there was incomplete evidence of cardiac derangement. The heart, therefore, was absolutely uninjured in only half the number. The patients remained in the Hospital for an average period of forty-one days. Bouillaud, who is the great advocate of bleeding in this disorder, and trusted to it almost exclusively, expresses his opinion that such complications are the rule, and not the exception. The second table gives the result of six cases treated solely with repeated doses of calomel and opium. In two subsequent classes were seen the effects of the same medicines aided by saline draughts, with and without nitre. The progress of the disease under each plan appeared to be much the same. The total of twenty-four cases presented six of inflammation of the heart or its membranes, of which two proved speedily fatal. The average number of days in Hospital under mercurial treatment was thirty-seven. The action of some reputed specific remedies was then considered. With regard to opium, reference was made to some tables published by Dr. Sibson in the *Association Medical Journal.* Twenty-one cases are here recorded, in which, when the treatment was commenced, the sounds of the heart were natural. Opium was given in frequent doses, sometimes as much as a grain an hour, besides other remedies supposed to have an effect in rheumatism. No less than fourteen of these cases, or exactly two-thirds, manifested while under treatment symptoms of valvular or pericardial inflammation. The cases, seven in number, treated with nitre alone, gave only one of cardiac complication. The average duration of the treatment was reduced to twenty-seven days. Further evidence in favour of nitre was deduced by comparing the result of cases treated with saline remedies alone, and those which had salines with nitre in addition. A table contributed by Dr. Basham to the *Transactions* of the Society was quoted. Of sixty-seven cases of acute rheumatism treated with large doses of nitre, the heart being in each case unaffected at the commencement of the plan, six only had symptoms of inflammation of that organ. In considering the treatment of saline remedies, the use of the term was limited in an arbitrary manner. It was assumed that the salts which potass and soda form with the vegetable acids undergo such changes in the system as to become equivalent, or nearly so, to the same quantity of alkali in combination with carbonic acid. Whether a certain quantity of potass is given as citrate, tartrate, acetate, or carbonate, the effect upon the urine and upon the system generally was held to be much the same. The arrangement was made accordingly. Those patients treated with an aggregate of such salts not reaching three drachms a day were considered as under saline treatment; those taking as much as three but less than four drachms, as under partial alkaline treatment; those taking from half an ounce to an ounce and a half, as under full alkaline treatment. Sixty-two cases appear to have been subjected to saline treatment, alone or with other remedies. These afforded a proportion of heart affection of 1 in 3.6. The conclusion was that salines in such quantities had but little influence upon the course of the disorder; when used in conjunction with more potent remedies, the result always corresponded with the observed effect of the additional medicines when used independently. With the increased doses, which the author distinguished as partial alkaline

treatment, no diminution of the heart symptoms was observed, although the disorder terminated in rather a shorter time. The full alkaline treatment was exemplified by two tables. It consisted in the administration of the salts which potass and soda form with carbonic and the vegetable acids, in quantities varying from half an ounce to an ounce and a half daily. Half a drachm of the acetate, with twice as much of the bicarbonate of potass, dissolved in the haustus ammoniæ acetatis of the Hospital Pharmacopœia, furnished an ordinary form of prescription. This was given every four or six hours, and sometimes made to effervesce by the addition of a little citric acid. Salts of soda were sometimes resorted to. The total of forty-eight patients thus treated passed through the dangers of the disease, with only a single instance of any cardiac affection. In the exceptional case the murmur came on within twenty-four hours of the commencement of the treatment, and did not prove permanent. The average number of days in hospital, when this treatment was applied simply, was twenty-five, the smallest of all; when other medicines, as colchicum, were used in addition, five days were added to the average period. Dr. Garrod's published cases, in which bicarbonate of potass was used alone, were quoted as rather less successful than those at St. George's, in which neutral salts were given in addition. Twenty-four of Dr. Garrod's cases afforded three of inflammation of the heart or its membranes. It was concluded that the carbonates of potass and soda, and those of their other salts which in the body are capable of being converted into the carbonates, exert an especial curative power in rheumatic fever, and, if given in time, will completely protect the heart from the dangers by which it is surrounded. Taking the proportion of heart affection under the alkaline system, one in forty-eight, and, with this as a standard, reviewing the other plans of treatment, the result was striking. One hundred and thirteen cases where other remedies were used gave thirty-five instances of cardiac mischief, or a proportion of 1 in 3.2. Nitre, next to the alkalies, was the most successful. The general symptoms were shortened under its use, and the frequency of cardiac inflammation was reduced to 1 in 10. Regarding the other remedies which have been credited with the cure of acute rheumatism, it simply became a question which were useless and which injurious. Mercury allowed a proportion of cardiac inflammation of 1 case in 4. Saline treatment gave a worse result. With bleeding one-half of the cases became thus complicated. Under opium the mischievous influence of the disorder attained its maximum. Two-thirds of the cases so treated had the symptoms of endocarditis or pericarditis. With the exceptions stated, it was maintained that the more active the remedies the more untoward, generally speaking, is the progress of the disease. It was shown that the use of colchicum retarded the recovery of the patient. The practical deduction was, that acute rheumatism is best treated by giving at short intervals a solution of nitrate, acetate, and bicarbonate of potass in such doses that ten or twelve drachms of the two latter salts together are taken in the twenty-four hours. Half a drachm of the acetate, with a drachm or a drachm and a-half of the bicarbonate, and ten grains of nitre, would answer the purpose. A brief review of the history of the alkaline treatment of rheumatism concluded the paper.—*Med. Times and Gaz.*, June 21, 1862.

27. *Veratria in Rheumatic Fever.*—In a clinical lecture recently delivered by M. BOUCHUT at the Hôpital Ste. Eugénie, the employment of veratria in the treatment of rheumatic fever in children is strongly advocated. M. Bouchut, whose opportunities for testing any novel method of medication are so extensive, comes forward with thirty cases of acute rheumatism, in which veratria has proved sufficient for the cure of the malady in from three to twelve days, the amelioration setting in on the second or third day of its administration. One of the most remarkable effects of the action of veratria in rheumatic fever is the rapid fall of the pulse; whatever its frequency, the diminution under the use of this drug is most marked, the pulse falling to sixty or even fifty, becoming almost thready and imperceptible, without causing to the patient any feeling of malaise. This abatement of the circulation M. Bouchut considers as the signal for the diminution of the doses. With regard to the effect of veratria on heart complications, this is probably indirect and due to the annihilation of the rheu-

matic poison. Besides its influence on the pulse, this medicine may, if given in too large quantities, irritate the mucous membranes, and produce vomiting, diarrhœa, and colic. The doses should therefore be small, and the ordinary formula recommended is, veratria and extract of opium one grain each, to be divided into ten pills, of which two pills are to be given the first day, three the second, four the third, five the fourth, and so on, increasing one pill each day until the condition of the pulse or the irritation of the mucous membrane compels a diminution. The difference of price in hospital practice between the use of veratria and that of sulphate of quinia, is of course one point of comparison in which the advantage remains very decidedly with the former; add to which, in a therapeutical aspect, veratria is perfectly innocent of those charges of exciting rheumatic determination to the membranes of the brain which in some measure appear to have been justly adduced against quinia since the employment of large doses of this drug has come into fashion.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

28. *Iliac Aneurism remedied by opening the Sac, and tying the Common Iliac, and the Internal Iliac Arteries.*—Professor JAMES SYME, of Edinburgh, communicated a case of this to the Royal Medical and Chirurgical Society (May 27, 1862). He reminded the society that ten years ago he had endeavoured to show reason for regarding the preference which had been given to the Hunterian operation for aneurism as too exclusive, since the circumstances which rendered it so advantageous in cases where the popliteal artery was affected might be so different in other situations as to reverse the grounds of choice—as in axillary aneurism, where the vessel is easily reached at the seat of rupture, and with great difficulty, as well as danger, tied above the clavicle. In reply to the doctrine usually taught, that the artery is not in a condition suitable for the ligature so far as the aneurismal sac extends, it was remarked that the size of the tumour does not depend upon the state of the vessel, and therefore cannot be taken as any measure of the extent to which its coats are impaired; while the formation of a sac, so far from proving injurious to the artery, must rather tend to strengthen and support it by consolidating the textures in its neighbourhood. Trusting to this view of the matter, the author had operated with success in various cases, and now desired to relate one of recent occurrence, which seemed strongly corroborative of the opinion he had expressed. R. L., a seaman, aged 31, towards the end of November last received a blow on his left groin, which caused a painful swelling that was treated as glandular; and about a month afterwards strained himself on the same side in leaping from a ship that was about to sink, with the effect of causing the formation of another tumour some inches higher up than the former one. On this account he repaired to the Cumberland Infirmary, at Carlisle, and was then found to suffer from an iliac aneurism. After remaining a week in the hospital, he went home until the end of February, when he was admitted into the hospital of Dumfries. Operative interference having been there declined, the patient again went home and betook himself to intemperate habits, with the effect of injuring his health and greatly increasing the tumour. On April 18 he was admitted into the Royal Infirmary of Edinburgh, when the aneurism was found to extend from below Poupart's ligament considerably higher up than the umbilicus, and from two inches beyond the middle line of the abdomen towards the right side, completely across the left iliac region, so as to overlap the crest of the ilium. There was a strong pulsation throughout the whole extent of the tumour, great pain in the course of the crural nerve, and considerable œdema of the thigh. On the 20th, chloroform having been administered, the cavity was examined by introducing first one finger, then another, and finally the whole hand, without any trace of the artery being detected, whence it was concluded to be out of its usual situation. A screw clamp